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By E. Stuhlinger

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Ernst Stuhlinger¹

Since its inception, space travel has met with the problem of carrying out technical developments before research had created the appropriate scientific foundations for them. This problem is not new. During the course of history many technical projects were started and successfully accomplished, before science could contribute an exact knowledge of their details. Gunpowder, photography, and airplanes are examples of such developments. Other projects, however, such as uranium nuclear reactors, have their origin in basic and exact research. Each step was prepared carefully and according to plan by scientists, before the engineer started his technical projects.

In space travel we find examples of both methods. The problem of protecting a space object from burning at re-entry into the atmosphere, was technically solved without our comprehending the laws which govern boundary layers, heat transfer and heat conduction at high temperatures. Other projects, such as the development of electrical propulsion systems, go through a long phase of research and laboratory work; the first electrical rockets for space flights will not be constructed before we are familiar with the basic principles of the system in all their details.

The space travel engineer prefers the second method of development, i.e. the careful preparation of technical projects through research, since this represents the natural, more reliable and generally also less expensive way of development. In spite of this he will sometimes choose the first method of development and justify the higher cost, the greater risk factor and the possibility of a dead-end development, by lack of time or often by giving political reasons.

Apart from the course taken with respect to particular development projects, all space travel engineers agree on the fundamental importance of basic research. In America there are presently two great national groups interested in space travel: the National Aeronautics and Space Administration (NASA) and the Air Force. The Assistant Secretary of the Air Force said recently in a speech: "Although we have at the moment no military assignments in space, the Air Force spends 750 million dollars yearly for space research in order to prepare for a still unknown future".

¹George C. Marshall Space Flight Center, National Aeronautics and Space Administration, Huntsville, Alabama

It is difficult to say how great an amount NASA spends yearly on space research projects - not including development projects - but it can probably be said that NASA spends more money on research than the Air Force.

The great variety of space travel research problems can be divided into three categories:

- 1) Problems which must be solved before the completion of existing development projects.
- 2) Problems whose solution will improve and further the existing projects.
- 3) Problems which belong to the category of basic research in preparation of future projects.

Examples of the first category are given in Table I. Problems of this kind are almost exclusively recognized by those persons who participate actively in development projects, that means by engineers and by project leaders of industrial and governmental development centers. The carrying out of these research tasks should be placed in the hands of these same men. They are intimately familiar with the problems; they can best judge the value of a proposed solution; they will test a solution most quickly and most reliably; and if it proves successful, they will apply the solution most rapidly to the development project. The financing of these research assignments should come from a fund allocated to every development project as a "Research Fund". Small development projects should receive 5 percent of the development cost as research fund, big projects should receive 2 or 1 percent of their development cost as research fund.

Examples of the second category are given in Table II. These problems are still related to developmental projects, but they go beyond the limits of present projects into the future. They are especially recognized by project leaders and program directors in charge of the definition and control of present developmental projects. While they are working on the details of a project, they foresee the coming generations of projects. They see the holes in our knowledge, the limits of our capabilities. These research problems are also recognized, however, by those scientists and engineers who observe and pursue the developmental projects from a certain distance, by not taking an active part in them. Both groups of men are in charge of the task of establishing research programs which make full use of our present technology. The formulation of these research programs should be restricted to broad outlines which leave the researcher considerable freedom in the choice of his means and methods, even of his ways and objectives. Inspirations and contributions to this program will come from industrial and governmental development centers. Industry, national centers and universities should be in charge of carrying out such a program. The financing should come from a "Fund for Applied Research".

Examples of the third category are given in Table III. They belong to basic research whose primary goal is to broaden our knowledge of nature. It is obvious that the results can be applied to space travel projects, but these problems are not studied with the intention of supporting a certain given developmental project of space travel. Research work of this kind originates in the ideas of individual researchers, not in the controlled programs of development leaders. Universities, research institutes, industrial research laboratories and in some cases also research divisions of developmental centers are the homes of basic research. The only help which the State can give at first is the possibility of good training and later the possibility of free and undisturbed research. The financing of this basic research should be taken care of in part by the departments of education and in part by industry as a contribution to the cultural life of a nation.

The above presentation of space travel research problems is the result of experiences gathered during the past years in America. It should not be difficult, however, to find in this picture a place for those nations who cannot carry out such gigantic development projects as cosmic satellites or Saturn rockets.

Research assignments of the first category, which must mainly be solved during the carrying out of a given developmental project, will essentially remain in the hands of the developmental groups. Assignments of the second category, however, are not tied to certain developmental projects; they challenge the cooperation of other research centers, even of other countries. The great space travel programs, at least those of America, are so much open to the public that an attentive observer can easily recognize the plans, the progress, and above all, the problems whose solutions are insufficient or nonexistent. Table IV shows some of these problems, which can be worked on by smaller research laboratories with relatively modest means. Fortunately, such space research problems find increasing attention in several European countries.

Out of many research tasks four typical problems will be described here in more detail. The first concerns an improvement of our technological know-how. The next two belong to basic physics, but their solution will have an immediate influence on the advance of space travel. The fourth example is a problem of basic research whose solution would have a tremendous effect on physics, on space travel, and above all, on the whole field of engineering. Considerable contributions to the solution of these problems can be furnished without resort to costly space travel developmental projects.

The first example is an electrical rocket system, a so-called ionic rocket. A laboratory model is shown in Figure 1, a schematic drawing in Figure 2. Concerning the development of this system two components proved difficult: the ion source, which must furnish a current of great density

(20 to 30 ma per cm^2) and of high yield (over 90%), and the neutralizer, which must neutralize the ion current after it leaves the accelerator chamber, and this must be done down to the small fraction of one percent by adding electrons, whereby charge density and current intensity must disappear. The work on both problems has already exceeded years ago the limits of a simple technical development and has entered the research fields of surface physics and plasma physics. Solutions which can be used have been found, but they need further improvement.

The second example is meteorite research. The frequency of occurrence of meteorites in proximity of the earth and of the moon, and in the interplanetary space, as a function of their mass and velocity, is a problem which interests astrophysicists, as well as space travel engineers who must give their space vehicles sufficient protection against meteorite collisions. The frequency of occurrence of meteorites as a function of their mass is shown in Figure 3. Whereas the distribution curve is rela-

tively well known below 10^{-7} g and above 10^{-2} g, there is an uncertainty factor of 100 to 1000 between the two. Our knowledge of the penetration depth of meteorites of given mass, density and velocity, into metal layers is similarly uncertain. Theory prefers the hydrodynamic model of crater formation, but an experimental confirmation of the theoretical results has not yet been possible. In order to study meteorite effects it is very

desirable that methods be developed with which particles of 10^{-7} g to 10^{-2} g mass can be accelerated in laboratory tests to velocities of 30 or 40

km sec^{-1} .

The third example concerns the solar flares or plasma eruptions of the sun surface, which are accompanied by an ejection of energetic protons and also by intense flare appearances on the face of the sun. The flares originate in the vicinity of solar spots, i.e. in places of high magnetic field intensity. Because of its high conductivity, the ejected plasma cloud draws the magnetic field along and keeps it enclosed as in a "magnetic

bottle". While the cloud expands at approximately 2000 km sec^{-1} , the protons spiral with almost light velocity around the magnetic field lines (Figure 4). The plasma cloud reaches the area of the earth path after about 24 hours. At this time a high intensity proton radiation reaches the earth, and fades out after a few hours. On the earth surface the proton radiation is only noticeable by secondary neutrons. The screening effect which the magnetic field of the plasma cloud exerts on energetic rays is known as the Forbush effect.

Figure 5 shows the distribution of solar flares over the past seven years. It is clearly recognizable that they accompanied the occurrence of solar spots, whereby the flare minima coincided with the minima and maxima of the solar spots. The most intense proton eruption known to

date occurred in February 1956; the second most intense in November 1960. The next maximum activity is to be expected around 1967-1968, a time which unfortunately coincides with the planned date of the first manned moon flight. The danger of solar flares for manned space flights must not be underestimated. While a dose of a few hundred rem seems permissible for men, the 1956 flare would have meant a dose of 2000 rem for the unprotected space traveller. An effective radiation protection against radiations of such intensity is too heavy for present space vehicles. Fortunately, flares of such intensity are very rare, and the probability of their appearance during the few days of a moon flight is small. The finding of relationships between flares and other sun effects which occur a long time ahead of the flares would be of greatest importance. The Russian astrophysicist Severny of the Crimea Observatory developed a theory according to which flares occur always when opposite poles of two solar spot pairs meet in their motion across the sun surface. The magnetic fields then mutually cancel each other, and the energy which was stored in them causes a plasma cloud to erupt.

The fourth example of a research problem has been for a long time the object of intensive studies: the transformation of energy, and essentially the transformation of heat into electricity. The problem is to find energy transformers of small weight but high efficiency. Although nuclear fission supplies heat sources of high intensity, all energy transformers suffer from too much weight and too low efficiency; these disadvantages are especially undesirable in space vehicles. At present, development and research concentrate their efforts on rotating systems (turbo-generators), thermo-elements, thermionic transformers, solar cells and dynamic plasma transformers. The ultimate in energy transformation is the annihilation of matter radiation in accordance with the well-known

Einstein equation $E = Mc^2$. The significance of an energy source of this kind will be shown with the help of a rocket system. In each rocket system, be it chemical, nuclear, electrical, or in a hypothetical photon system, we can define an initial total mass M_0 , a final mass M_e and a

propellant mass M_p (Figure 6). We now divide the propellant mass M_p into a part γM_p , which is equivalent to the total energy E contained in the propulsion jet:

$$\gamma M_p = E/c^2$$

and into the remainder $(\gamma - 1)M_p$, which is equal to the total ejected mass. This consideration which can be applied to each rocket system leads to the simple rocket equation

$$\frac{M_0}{M_e} = \left[\frac{1 + u/c}{1 - u/c} \right] \frac{1}{2 \sqrt{2\gamma - \gamma^2}}$$

u = final velocity
c = light velocity

This equation is also strictly valid for relativistic velocities and is essentially based on the work of Professor E. Saenger.

The factor γ marks the fraction of the total propellant mass which is transformed into energy. In all known rocket systems this factor is extremely small; Table V shows the γ factors for rockets as they can be built today or at least as they can theoretically be designed today. An "ideal" fission or fusion system is in a position to transform the total energy freed by a lossless nuclear reaction into kinetic energy of the propulsion jet.

The Table shows that even hydrogen fusion does not yet represent transformation of matter into energy in macroscopic quantities. Processes of this kind are still completely unknown today; and the research problem of matter energy transformation will certainly remain the unfulfilled wish of physicists, engineers and space travellers for a long time to come. If this problem were only solved in a small measure, rocket systems of tremendous capacities could be built, as shown in Figure 7. A rocket with a transformation factor $\gamma = 0.5$ and a mass ratio 10:1 could reach the nearest fixed star in a flight of about 15 years duration, provided that after the solution of the energy transformation problem all other technical problems can also be solved.

When the factor γ reaches the value 1, the rocket system becomes a pure photon rocket.

The last Table shows a few fields of space research, presented in increasing order of difficulty. As a coworker in the great and very active American space travel program which has led us in the past years to an ever-increasing number of research problems, I would like to mention, finally, an experience which we go through again and again: the majority of the research problems will not be solved by any amount of money, nor by the size of a rocket project, but only by the work of a persevering and dedicated small group of researchers, who often work in a laboratory of modest size. These groups which are filled with the desire to increase our knowledge, familiar with the problems of science and engineering, supported by an understanding administration or a far-seeing industry leader - and which do not exist only in Russia and America, but in almost every country - these groups are our most important aid in the exploration of space.

TABLE I. RESEARCH PROBLEMS OF PRESENT PROJECTS

Mechanical properties of big space vehicles
Proofing and lubrication materials for extreme temperatures
Burn-off stability in big rocket engines
Electronic devices of high sensitivity and long life
Communications devices of great band width
Aerodynamics in the hypersonic field
Wind intensities at high altitudes
Meteorite occurrence frequency in proximity of earth and moon
Physics of the high atmosphere
Physics of the moon surface

TABLE II. RESEARCH PROBLEMS OF THE NEXT PROJECT GENERATION

Effects of long stay in high vacuum
Biological effects of weightlessness
Optical navigation methods
Methods of computing planet orbits
Production of electrical energy in space
Fuel cells of a high degree of efficiency
Natural and artificial radiation in space
Nuclear reactor at high operating temperatures
Electrical rocket systems

TABLE III. PROBLEMS OF BASIC RESEARCH

Mathematical methods for the solution of celestial mechanics problems
Superconductor physics and techniques
Effects of natural and artificial radiations
Transformation of matter into energy
Solar physics
Structure of moon and planet surfaces
Galactic and outer-galactic radiations
Relativistic effects

TABLE IV. RESEARCH FIELDS FOR LABORATORIES

Gases and solids at extremely high temperatures
 Fatigue phenomena of materials under extreme conditions
 Light metals of high strength
 Very compact electronic devices
 Millimeter waves for navigation and guidance methods
 Laser physics and technology
 Solar batteries of high yield
 Plasma physics
 Reaction kinetics of dilute gases
 Ion sources of high intensity and long life

TABLE V. MASS-ENERGY TRANSFORMATION FACTORS OF SOME ROCKET SYSTEMS

<u>System</u>	<u>Factor γ</u>
Chemical ($O_2 + JP$)	5×10^{-11}
Chemical ($O_2 + H_2$)	10^{-11}
Heavy current arc	10^{-9}
Ions or plasma	5×10^{-8}
Nuclear fission (U with H_2)	5×10^{-10}
Nuclear fission, ideal	10^{-3}
Nuclear fusion, ideal	4×10^{-3}
Pure photon rocket	1

TABLE VI. SPACE TRAVEL RESEARCH FIELDS

Space travel medicine
 Radiations, particles and fields in space
 Solar physics
 Lunar physics and planet physics
 Traces of life on planets and in space
 Cosmology and cosmogony
 Weather forecasting on earth